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# On Lavoisier's Achievement in Chemistry.

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*Abstract.* Methodological recommendations common during the last thirty years have not prevented the emergence of views which are arguably no less caricatured and incorrect than was previously the case, even when account is taken of the heavily biased, mainly nationalistic, accounts concerning Lavoisier from the century after 1835. This article considers many of the categories of Lavoisier's achievement in chemistry, considers some of the more startling issues in the recent historiography, including negative accounts by Bensaude-Vincent (1993), Siegfried (1988), Kim (2003) and Chang (2009, 2010, 2011, and 2012), and contributes towards a process of identifying a judicious view.

## 1. Introduction.

Whereas methodological recommendations common during the last thirty years, such as considering science as practice, considering disciplinary identity, investigating local scientific contexts, considering scientists other than central figures, and considering the total historiography for an issue (e.g. Abbri, 1989, p. 308), have led to some illumination, they have not prevented the emergence of views which are arguably no less caricatured and incorrect than was previously the case, even when account is taken of the heavily biased, mainly nationalistic, accounts concerning Lavoisier from the century after 1835. Some of the recent historiography might be held to unintentionally illustrate Bierce's witticism (1967) that 'success is the one unpardonable sin against our fellows'. Whereas some range of views on a subject might constitute a rich multiplicity, the extreme range in this case also indicates the presence of what may be characterised as significant misjudgements.

The present article is a contribution towards a process of identifying a well-judged view of Lavoisier's achievement in chemistry. Due to the word limit for the article, it does not cover issues concerning revolutions nor a 'founding father' of modern chemistry, nor concerning the degrees of utility of constructivist tenets in this field, nor issues in the 'chemistry of life', mineralogy and metallurgy, and so on; it is not possible to refer to the complete literature, and it is not possible to refer more than extremely briefly to some of the philosophical issues. It is proposed that one advantage of considering Lavoisier's particular scientific achievement separately from general considerations concerning the nature of a scientific revolution is that there will be less likelihood of preconceptions concerning the latter railroading the understanding of the former.

'Accuracy' is a term open to much controversy, not least in the context of a scientific theory which will change and develop, including identifying smaller-scale entities within what at a greater scale are 'single' entities, and including such aspects as the presence of ions in liquids. In this paper, 'accurate' will be used to denote a theory that, at the level of what are the currently-recognised chemical elements and their compounds, was at the time of development and has remained an accurate part of the theory, well related to what it represents.

In this context, 'achievement' is used to imply the origination of items which were seminal to the development of, and still remain at some level of approximation part of, the current chemistry which has a high achievement in terms of the epistemic virtues. Manifestly such assessment of achievement is

inevitably partly presentist, but it also takes into some account the interim history of the science, and the historiography.

## 2. Air, oxides, and acidity.

The most fundamental early aspect of Lavoisier's achievement was his theory (Berthelot, 1890, p. 80) that air is not one of four indivisible elements (earth, air, water and fire), but is a mixture, of which the major components are what Lavoisier termed 'simple substances' (in compositional terms, with the exception that their states were affected by a cause which might be a substance). A noteworthy stage in the development of this theory was his realisation based on experiment recorded on 29 March 1773, that 'not all the air we breathe enters into metals being calcined, but only a portion, which is not found abundantly in a given mass of air' (Berthelot, 1890, p. 236). He developed the view that three of the then-known gases – then known as 'dephlogisticated air' (now oxygen), 'inflammable air' (now hydrogen) and what he was to call azote (now nitrogen), were 'simple substances', while other gases including 'carbonic acid' and 'nitrous air' were compounds. Whereas these aspects of his theory were not possible to establish fully at the time, they were integral to the subsequent fertile development of chemistry, and have subsequently proved to be accurate.

By contrast Priestley during the 1770s retained the view that air is an element, and theorised that air was transformed by phlogiston. His views on the latter process varied: he firstly (1774, p. 263) claimed that 'acid air' (now anhydrous HCl) and phlogiston were the only constituents of 'common air', in that (he claimed) the two together formed 'inflammable air', which could in turn be transformed into respirable air. However, he later (1775, p. 392) proposed that there was a 'regular gradation from dephlogisticated air, through common air, and phlogisticated air [now nitrogen], down to nitrous air', the common basis of them all being the nitrous acid (now  $\text{HNO}_2$ ). With regard to phlogiston he stated (1774, p. 260) that it 'has never yet been exhibited by itself in any form'. He finally (1788) abandoned the view that air was an element and backed Cavendish's 1784 theory that it was water that was an element, and added the theory that water was a component of all gases. However, they and Kirwan could not agree on a common view (McEvoy, 2010, 122) for the gases released from water: 'dephlogisticated air' now became 'dephlogisticated water,' while 'inflammable air' was 'phlogisticated water' – or might still be phlogiston itself. The original version of phlogiston had been non-isolable and not directly observable; the attempt to equate it with 'inflammable air' was abandoned by Kirwan due to an excess of problems, but was partially and not consistently retained by Priestley. Accordingly in the final theory it remained unclear what phlogiston was and how it could even be indirectly observed; these two aspects hamstrung the further development of the theory: to take basic examples, it was not clarified how common air or nitrogen or nitrous air or 'acid air' or 'alkaline air, etc., were supposed to be derived from or partly composed of water. Priestley's theories subsequently proved to be inaccurate.

That Lavoisier's specific experimental achievement concerning calcination was to be the first to exhibit that weight gain is equal to the weight of the breathable part of air absorbed, was already recognised by Kirwan (1789, pp.3-4). Kirwan's view was sufficiently exacting to avoid trespassing on the priority of Jean Rey in terms of hypothesising that the weight gain of metals during calcinations was due to the fixation of air (although that hypothesis had largely been forgotten, and was unknown to Lavoisier), the priority of the more widely known hypothesis of Stephen Hales that many bodies contained air 'fixed' within them, of Black in beginning to identify different gases so fixed (although Lavoisier was apparently initially unaware of Black's work), (Guerlac, 1961, pp. 11-23) of Guyton de Morveau in

determining that all metals gained weight when calcined, and of Bayen in tentatively suggesting that the weight increase of calces was due to the fixation of the elastic fluid described by Lavoisier in his *Opuscules physiques et chimiques*. (Poirier, 1998, p. 73) .

On this basis, Lavoisier developed the theory that metals were simple substances, that calces were compounds of metals and oxygen, and that the different compounds formed by a single metal or other simple substance, and which gave off different amounts of oxygen when heated, just contained differing amounts of oxygen. He could easily explain reduction without added charcoal in terms of relatively low affinity of some metals for oxygen, making their calces easy to reduce (Pyle, 2001, p. 111). This theory was again not provable at the time, was fertile in terms of predicting additional compounds of the simple substances with oxygen, and has been seen to be largely accurate.

Stahl's original theory, that phlogiston was added to calces to produce or 'revivify' metals, had led to an obvious problem in terms of explaining the known weight gain of the calces relative to the metals in the reverse process; the various possible hypotheses for this all proved generally unacceptable: (c.f. Pyle, 2001, p. 108); also the phlogistonists had greater difficulty in dealing with reduction without addition (Pyle, 2001, p. 109). Once again the later phlogistonists did not manage to arrive at a consistent theory; Cavendish and Kirwan theorised that calces contained water; Priestley managed to convince himself of this, even though he knew that it was oxygen that was contained in red calx of mercury, which he then had to theorise was a special case. The confusion that this kind of inconsistency caused was shown in that the reduction of red calx became the experiment around which debate in Germany polarised (Hufbauer, 1982, 118-144), and the failure to produce significant water during reduction of red calx was one factor in the widespread abandonment of phlogiston theory in Germany. Additionally, both the versions of phlogiston theory can be seen to be inaccurate.

Concerning acids, it is relatively rare to find recognition in the historiography that Lavoisier achieved the view that for many substances that are components of acids, differing proportions of oxygen could be combined with the substance, and acidity increases as the proportion of oxygen in the acid increases. This was demonstrable at the time and has proved accurate. It was therefore rational that he should hypothesise that oxygen was the cause of acidity; although that inference has proved inaccurate, it remains the case that nearly all then-known acids contained oxygen. To take an example of the eventual fertility of the theory, while Lavoisier was wrong to suppose that 'muriatic' acid (hydrochloric, HCl) contained oxygen, there are four oxoacids based on chlorine – hypochlorous HClO, chlorous HClO<sub>2</sub>, chloric HClO<sub>3</sub> and perchloric HClO<sub>4</sub> acids, with K<sub>a</sub> being approximately 10<sup>-7</sup>, 10<sup>-2</sup>, 10<sup>3</sup>, and 10<sup>8</sup> respectively (Monroe and Abrams, 1985, p. 41). The result was that the inaccurate parts of the theory could be removed without general damage to the fertility of his compositional theory, with the effects being largely limited to confusion concerning specific acids which did not contain oxygen. Once again by contrast, phlogiston theory did not include a consistent theory for acids.

Guyton commented in his second foreword in the *Encyclopédie Méthodique* (p. 633-4; Holmes, 1995, p. 46) that a great advantage of Lavoisier's theory was that 'we are no longer reduced to take for an explanation the fiction which identifies bodies as diverse as vital air, the toxic air of nitrous gas, inflammable air, and fixed air, and which makes the enormous differences between them depend on no more than a little more or less phlogiston'.

Siegfried (1988) alleged that among the 'central assumptions' of Lavoisier was oxygen as the principle of acidity; however, he had early on noted (Siegfried and Dobbs, 1968, p. 281) that oxygen was seen as important by Gregory (1806-7) not because it provided a theory of acidification, but because it allowed phlogiston to be discarded as a component part of certain bodies and the correct order of

simplicity to be established. Siegfried in 2002 retreated from his 1988 view, and returned to a view close to that of Gregory; this view entailed that the incorrect part of Lavoisier's theory of acidity, although noteworthy, did not affect Lavoisier's fertile compositional theories.

Bensaude-Vincent recognised (1993, p. 152) that Lavoisier's theory included that the degree of acidity depended on the oxygen content of the acid, but nevertheless indicated (1993, p. 155) that the oxygen theory hardly survived its creator. Chang repeated Siegfried's 1988 claim,. (2009, p. 241; 2012, p. 8) that the oxygen theory of acidity was a 'major pillar' of Lavoisier's theory. Concerning calcination, while most modern authors have acknowledged or not questioned Lavoisier's achievement and the superiority of his theory over that of Stahl, Chang (2009) did not do so, only noting that the phlogistonists had previously recognised the similarity of calcination and combustion, a point so uncontroversial that it had been explicitly commended by Lavoisier himself in his 1785 paper on phlogiston.

### 3. Combustion and heat.

Concerning combustion, as with calcination, Lavoisier's achievement was to state that during combustion, the breathable part of atmospheric air combines with the burning body (Thomson, 1802, v.1, p. 596); this has proved largely accurate. However, there were problems with his theory of the emission of heat and light during combustion: oxygen often unites with bodies without the extrication of heat and light; there was an explanatory problem when oxygen is in solid or liquid form and violent combustion takes place; and gunpowder was an anomaly within the theory, 'for the caloric and light must be supposed to be emitted from a solid body during its conversion into gas, which ought to require more caloric and light for its existence in the gaseous state than the solid itself contained' (Thomson, 1802, v.1, pp. 599-601). Lavoisier made attempts to deal with the anomaly, but without success.

In contrast to Lavoisier's partly-accurate theory, Cavendish's 1784 theory was considered to imply that combustion (like calcination) involved absorption of water, which has proved inaccurate.

Lavoisier's theory of heat from 1781 involved the central concept of 'the cause of heat'; this was his consistent definition of the later label 'caloric'. His *Traité* explained that this expression 'possesses this .. advantage, that it accords with every species of opinion, since .. we are not obliged to suppose this to be a real substance; .. so that we are still at liberty to investigate its effects in an abstract and mathematical matter' (Lavoisier, 1789, p. 5). In fact, all that was necessary was to assume some repulsive force that separates the molecules of matter. During his work with Laplace (1784 p. 4) they had stated that 'some suppose that caloric .. is merely a property of matter, and that it consists in a peculiar vibration of particles; others .. think it is a distinct substance'. Their experiments (1784 p. xi) had been formulated to be independent of the theory one preferred, and they put forward that 'we shall not decide in favour of either of these two hypotheses .. perhaps both are true simultaneously' (Fox, 1971, pp. 29-30); Boerhaave had previously held views in which heat depended both on a material substance and on motion (Fox, 1971, pp. 12-13). Until 1781 Lavoisier had had a consistent material theory of heat, but after that date his theory broadened to include physical explanations (Morris, 1972, p. 9). Whereas it is clear that Lavoisier preferred to think in terms of heat as a material substance, he was never able to provide a positive demonstration of its existence (Morris, 1972, p. 31) and needed to retain the view that it might be hypothetical (Lavoisier, 1785); this was something more than 'mere lip-service to epistemic prudence' (Chang, 2003, p. 910).

There were generic differences between caloric and phlogiston, not fully recognised by Lavoisier himself nor by such historians as Morris. Firstly, they had differing functions in their respective theories;

caloric was involved in functions for which particle motion supplied an alternative explanation to that supplied by a type of substance – that is, it was used to explain the state and spatial arrangement of a substance, but it was not otherwise used to explain chemical composition, nor the compositional results of reactions between chemical elements and compounds. By contrast, phlogiston was at the centre of the later phlogistonist compositional theory for recognisably different gases, explaining how they were produced from water, (as well as of the early phlogiston theory that metals were compounds). Secondly, caloric was always imponderable, whereas differing versions of phlogiston theories existed with it imponderable, heavy or of negative weight; disadvantages occurred with each version of the theory. All this meant both that the sub-theory of the cause of heat being a substance did not affect Lavoisier's practice of compositional analysis in which the initial and eventual substances involved in a chemical reaction were analysable in terms of weight, and that the sub-theory could be discarded smoothly. By contrast, phlogiston (in most of its variants) did preclude analysis in terms of weight of chemical reactions in which it was supposed to be involved, and it was impossible to discard from the theory dependent on itself. Lavoisier's theory concerning metals being 'simple substances' has proved accurate.

Concerning caloric, Thomson correctly quoted Lavoisier's 1789 definition (1802, v.1, p. 423) and 1784 statements (1802, v.1, p. 424); Butterfield (1949, p. 220) commented that 'caloric proved to be easily detachable from his system later'; Fox (1971, p. 22) commented that at this period 'the vibrationists as a whole seemed unable to relate the wealth of new discoveries in heat to their own theory, while the calorists by contrast rose most impressively to the challenge'; Beretta (1993, p. 209) emphasised the 'huge' differences between the concepts of caloric and phlogiston; Kitcher (1993, p. 278) quoted part of the passage from the *Traité*, and noted the need to beware of assumptions that Lavoisier's theory encountered problems due to the caloric theory.

However, many modern authors have been 'unwary' in this respect. Bensaude-Vincent (1993, p. 15) claimed that caloric and phlogiston were 'equally chimerical' and Chang (2012, p. 10) has followed this line. Bensaude-Vincent (1993, p. 300) and Kim (2003, p. 327) noted that if heat were material, in Lavoisier's theory neither it nor (the ponderable base of) oxygen could be isolated; however, Lavoisier's compositional chemistry of solids was unaffected by this issue, and he allowed for the possibility that heat was motion rather than a separate substance, in which case this difficulty disappeared for other states as well. Chang did not include Thomson's correct quote of Lavoisier's definition of caloric as 'the cause of heat', alleging (2009, p. 243) that 'by the 1850s, Lavoisier's basic picture of the universe was in tatters; later the kinetic theory would fill in the theoretical background', and (2012, p. 7) wrongly quoting caloric as 'the matter of heat'. Chang's (2009 and 2012) dismissal of caloric appears inconsistent with his (2003, pp. 906-7) list of its successes. Siegfried (1988) omitted Lavoisier's explanation of the change which the burning body undergoes, inaccurately implying that Lavoisier's whole theory of combustion was later discredited; Kim and Chang followed this line.

#### **4. The composition of water and consequences for theory.**

The most dramatic turn-around between the fortunes of Lavoisier's theory and phlogiston theories centred on the composition of water. Lavoisier was among the earliest to attempt to find what would be produced by the combination of inflammable air and vital air, for example with experiments during March 1774 and April 1775, but he designed his experiments relative to the hypothesis that he would find an acid: he collected the results over a water trough thus failing to identify the product of combustion (Berthelot, 1890, 111). During the time that his research on this topic was thus hamstrung, English experimenters

progressively moved towards a conclusion finally demonstrated by Cavendish in 1783 and communicated to Lavoisier by Blagden, that the product was water.

However, the phlogistonists had previously proposed (Cavendish, 1766) that when a metal is dissolved in a weak acid and hydrogen is released, the latter came from the metal such that it was likely that hydrogen was phlogiston. Lavoisier now countered that the metal was taking oxygen from the water, resulting in the release of the hydrogen, and this was shown experimentally when Priestley repeated the experiment over mercury not water. Lavoisier (1782, 509) was also able to clarify that when sometimes metal is dissolved in an acid and released fumes of 'nitrous gas' or 'volatile sulphureous acid', this was being released as oxygen was taken from the acid, and that this outcome was more likely, the stronger the acid being used.

Lavoisier announced the theory that the two gases concerned were simple substances; initially there was widespread refusal among phlogistonists to accept that Lavoisier's decomposition and recomposition of water demonstrated his theory, but this theory was another landmark stage resulting in the fertility of his compositional theory, and has proved substantially accurate.

By contrast, the phlogistonists had problems and did not arrive at a consistent theory (c.f. Abbri, 1984, pp. 298-314; Beretta, 1993, p. 181). Cavendish's (1784) change away from his former view in response to Lavoisier's 1783 theory involved that water was still an element: water with a deficiency of phlogiston was oxygen, while 'inflammable air' was water enriched with phlogiston (or perhaps still phlogiston). Kirwan found Cavendish's theory incredible, in that hydrogen gas and oxygen gas would each be almost entirely water even before they combine to form what we experience as water; however, his own version of phlogiston theory, continuing the identification of 'inflammable air' with phlogiston, required that 'inflammable air' and 'dephlogisticated air' can form either water or fixed air, which others found incredible (Margolis, 1993, p. 63). The relative ease with which Kirwan's theory could be attacked, plus his failure to take the theory further, led to his abandonment of the theory in 1791. Cavendish's theory in the version with hydrogen as phlogisticated water could be defended at the time with regard to water, but no means was proposed for extending such a theory to other gases. Priestley's auxiliary hypotheses that all gases contained water and that several gases differed from water just via differing amounts of phlogiston were confused not least by his continuing inconsistent use of the view that 'inflammable' air was phlogiston.

Moreover, the potential difficulty of developing a phlogistic compositional theory that related to the compositional theory that was actually being developed became greater as the latter progressed. For example, if one compares Dalton's formulae for the composition of the gases known at the time with Cavendish's theory, there appears to be no method by which these can be translated nor made consistent. One attempt to represent hydrogen would give



which involves major problems, and the various possibilities for some modification of this representation still give problems. During the development of the periodic table, the theory that water was an element and that hydrogen, oxygen and nitrogen weren't elements and that all other gases contained water, plus the theory that none of the metals were elements but all metals were compounds containing phlogiston, would have been a crucial stumbling block and inconsistency. The sporadic later views as to what phlogiston might possibly have been, such as electrons, or energy (Odling, 1871), were inconsistent with Cavendish's theories that hydrogen was phlogisticated water and oxygen was dephlogisticated water. The above were among the reasons the phlogistic views on composition were infertile; the phlogistic views have also proved inaccurate.

The discovery of the composition of water, and its theoretical consequences, were perceived as crucial by those who became Lavoisier's collaborators in 1787. Berthollet noted this for example in a proselytising letter (May 4, 1785) to Guyton (Kim, 2003, p. 525). Guyton stated this in his second foreword in the *Encyclopédie Méthodique* (Guyton 1789, 625-34; Holmes, 1995, 18). Among historians, Berthelot (1890, p.122) stated, 'it is incontestable that Lavoisier was the first to see the theory clearly ... (and) who first dared to announce publicly and clearly the composition of water'; this was closely paraphrased by Partington. Chang demoted Lavoisier's achievement in this respect, proposing (2009, p. 244) that Lavoisier 'only' put a new theoretical interpretation on Cavendish's discovery and (2012, pp 52-3) noting Lavoisier's correct identification of where the hydrogen was coming from during dissolution of metals in acid, but in the rest of his presentation not accepting this as of central importance.

## 5. The new chemical nomenclature and the *Traité*.

The original aspects of the *Méthode de Nomenclature Chimique* (1787), relative to Guyton de Morveau's (1782) nomenclature of the neutral salts, included: (1) the method of naming being proposed to be more important than the specific nomenclature, since in principle with this method all future substances and compounds could be named consistently, and since semantic clarity in terms of the combination of words was an essential counterpart to chemical clarity in terms of the combination of simple substances (Perrin, 1988b p. 53; Beretta, 1993, pp. 187-206); (2) the inclusion of simple substances that are components of atmospheric air and of water; (3) concerning oxides, a classification for the first time largely correctly indicating the simple substances of which they are composed and indicating via suffixes relatively how much oxygen the particular oxide contained, in column III (Beretta, 1993, p. 210); (4) concerning oxoacids, a classification for the first time indicating via suffixes that the acid contained oxygen and relatively how much oxygen the particular acid contained, in column III; (5) for the neutral salts, a classification that for the first time managed to indicate via two words the three simple substances of which they were composed, and specified via suffixes that the salt contained oxygen and relatively how much oxygen the particular salt contained, in columns IV and V (Beretta, 1993, p. 210); (6) a partial 'map' of compositional chemistry on which nearly all the compounds included oxygen (Beretta, 1993, p. 212); (7) a very comprehensive dictionary including both old/new and new/old listings.

In effect, the *Nomenclature* began the demonstration of the fertility of Lavoisier's theory both in terms of 'consolidating' existing chemistry, that is of increasing its simplicity and consistency, after a period of proliferation of experimental results, and also in terms of providing a predictive method for identifying new compounds. The issue of the *Nomenclature* 'left the phlogistonists with the difficult if not impossible task of creating a phlogistic nomenclature that was equally expressive and effective' (Beretta, 1993, p. 216), a challenge to which none of them rose: their tactic of arguing for the old nomenclature, which had already been criticised by Macquer, Bergman and Guyton, was an inadvertent demonstration of the unfruitfulness of the theory.

The need for a new chemical nomenclature had arisen due to the ever-growing number of laboratory-prepared bodies which deserved individual names, and because in the absence of an overall coherent theory of chemistry they had been named without system. Initial steps towards a new chemical nomenclature had been taken by Rouelle, and Macquer (1766) had proposed a simple-to-complex compositional nomenclature for salts in his *Dictionnaire*, although he had relied on the traditional lexicon in his later works. A binomial nomenclature had been proposed by Torbern Bergman (1788), under the influence of Linnaeus, but had been published first in Swedish and then in Latin; Guyton de Morveau



(1782) had taken this idea forward in his nomenclature of neutral salts, published in French. However, none of these had yet been widely adopted, nor did they deal with the chemistry of acids.

Almost inevitably the *Nomenclature* had imperfections (Beretta, 1993, p. 219): as previously noted, the chart incorrectly included muriatic and fluoric acid as oxoacids; some of the oxides listed with different colours are now reckoned to not be distinct substances; and some of the gases listed do not contain oxygen. For the carboxyl acids, column I of the table listed a radical of these acids as not-yet-decomposed, which was strictly correct even if not fully consistent with the rest of the components of table I, nearly all of which were correctly identified simple substances.

Of the original aspects numbered above, (1) was due to Lavoisier's realisation, prior to the 1782 work by Guyton (Klein and Lefèvre, 2007, p.89), that the analytic method of Condillac's *La Logique* (1780) should be applied to chemistry (Albury, 1972, p. 277, and (2)-(6) derived their innovative content from Lavoisier's theoretical insights concerning simple substances and the composition of air, water, calces, oxyacids and salts.

In terms of the production of the *Nomenclature*, it appears that Berthollet's assiduous letter writing to Guyton paved the way for Guyton's visit to Paris during 1787 and for the joint work. Guyton's participation was honoured by deference to his choices of specific words in several cases: for example, the word calorique was introduced by Guyton in 1787, after his previous use of the word calorifique in 1785 (Partington, 1961, v. 3, p. 421; Morris, 1972, p. 2) in the context of a material theory of heat; Lavoisier would apparently have preferred the word 'thermogène' (Morris, 1972, p. 2).

It is clear that among the co-authors, Lavoisier had the priority for the general ideas behind what was original in the *Nomenclature* relative to Bergman's (1788) and Guyton's (1782) versions, and as Beretta (1993, p. 185) pointed out, Lavoisier's lecture on the Condillacian method preceded Guyton's 'conversion'. However, Guyton had the priority with regard to interest and involvement in preparing a chemical nomenclature, albeit not including pneumatic chemistry, and his 'conversion' eliminated the most obvious potential competition (given that Macquer and Bergman had both died in 1784); it is clear that the impact of the *Nomenclature* gained considerably from the joint authorship including both men. In all these circumstances, it seems most reasonable to divide the majority of the credit for the *Nomenclature* between Lavoisier and Guyton, with a lesser share to Fourcroy (Klein and Lefèvre, 2007), and credit to Berthollet for setting up the joint authorship.

It is important to be clear concerning what the *Nomenclature* was not or did not do: (a) it did not constitute a sudden change from the old names, but was part (albeit the outstanding part) of a process which had been going on since Rouelle; if the joint *Nomenclature* had not been undertaken, one of the previous attempts from the 1780s might have been adopted. (b) It did not constitute a break following which the old names became untranslatable: care was taken to ensure translatability with the two dictionary listings, and there had in any case been no actual problem of translatability (Pyle, 2001, p. 104 and 117; Bird, 2000). (c) It did not force the abandonment of the old language: it gave scrupulous attention to the old language, and gave cogent reasons for the use of the new nomenclature – it didn't even force its own use, but led as Priestley (1800, p. 19) said to chemists forcing themselves to learn it in order to read new articles; and as Holmes pointed out (1989, p. 16) the old names still exist and nothing prevents their understanding or use. (d) It did not lead to the forgetting of the history of chemistry: among many examples, Davy undertook his 'extensive flirtation' with phlogiston, there has recently been increasing attention to the previous chemistry (e.g. Holmes, 1989; Kim, 2003; Simon, 2007) and several other writers have emphasised continuities and similarities between the previous chemistry and Lavoisier's chemistry (e.g. Perrin, 1987; Pyle, 2001; Klein and Lefèvre, 2007). (e) The *Nomenclature* did

not reflect a sudden change in theory: Lavoisier had been preparing his theory for a dozen years before the balance began to tip in favour of his theory, and the process of change took years; the proposal by Baumé, Cadet, Darcet and Sage that 'it is not in a day' that a theory that had reigned for a half-century would be overthrown was reasonable but not an accurate objection to the new system.

Concerning the new *Nomenclature*, Thomson (1830, v. 2, p. 133) stated that:

In 1796, or nine years after the appearance of the new nomenclature, when I attended the chemistry-class in the College of Edinburgh, it was not only in common use among the students, but was employed by Dr. Black, the professor of chemistry, himself, and I have no doubt that he had introduced it into his lectures several years before. This extraordinary rapidity with which the new chemical language came into use was doubtless owing to two circumstances: .. the very defective, vague and barbarous state of the old chemical nomenclature .. (and) the superiority of Lavoisier's theory over that of Stahl. The subsequent progress of the science has betrayed many weak points in Lavoisier's opinions; yet its superiority over that of Stahl was so obvious, and the mode of interrogating nature introduced by him was so good .. that no unprejudiced person, who was at sufficient pains to examine both, could hesitate about preferring that of Lavoisier.

Fourcroy credited the *Nomenclature* with the consolidation of Lavoisier's pneumatic theory (Siegfried, 2002, p. 189). Beretta (1993, p. 203) noted that Lavoisier asserted 'that an entirely new nomenclature will last forever, with only occasional minor amendments. It is striking to see that so far, he has been proven right, and that the language of inorganic chemistry still uses the words of the naming method he outlined in 1787'. Some interpretations (Christie and Golinski, 1982, p. 260; Bensaude-Vincent, 1993, p. 423) have over-estimated the changes, not taking into account some or all of points (a) to (e) above.

Concerning credit for the nomenclature, Smeaton (1957, p. 26) considered Guyton to have been only a collaborator, and Crosland (1962, p. 174) proposed that the chairman of the work was Lavoisier rather than Guyton, but Guerlac (1961, p. 80) claimed that the work was originally suggested by Guyton. Beretta (1993 pp. 186-239) gave ample evidence concerning Lavoisier's work being the impetus behind the original features of the *Nomenclature*. Kim only recognised Lavoisier's Condillacian input but none of the other matters in the *Nomenclature* that derived from Lavoisier's ideas, claiming that all the detail was due to Guyton; on this basis she claimed (2003, p. 391), that the *Nomenclature* was 'not Lavoisier's'. Chang (2009, p. 245) omitted nearly all aspects of the *Nomenclature*, only mentioning the naming of oxygen.

As McEvoy has noted (2010), several modern authors followed the constructivist line that scientific events are local while failing to illuminate why in that case the effects were so quickly perceived world-wide. Qualifications have been made concerning local variations in the perception and take-up of the new nomenclature (e.g. Bensaude-Vincent and Abbri, 1995), but Thomson's and Priestley's above quotes show the great impact perceived by some of Lavoisier's near-contemporaries.

Lavoisier's *Traité* (1789) included some further original aspects, continuing the fertile processes of the consolidation of existing chemistry, increasing its simplicity and consistency, and in terms of predicting new compounds. For example, the carboxyl acids with merely undecomposed radicals were separated from the acids whose radicals were simple substances; this produced a table of simple substances which was the precursor of the modern table of elements, and has proved accurate in nearly all

cases. Also, for the simple substances, for the first time a table was given for increasing oxygen content and indicating the different substances produced, both different oxides and acids of increasing levels of acidity (Crosland, 1973, pp. 318-9). This table was filled out in many cases by prediction rather than experiment, such that for example where different acids are just identified as grey and white respectively, the details are now in many cases known to be wrong; also several of the proposed metallic acids were not correct. However, while the levels of oxygenation are now known to be considerably more complicated, and the occurrence of compounds less regular, than the table puts forward, the basis for a graphic display of levels of oxygenation was established with this table.

However, the *Traité* exhibits very clearly why Lavoisier's work has been so difficult for historians to judge. On the one hand Fourcroy gave the excessive praise that the *Traité* was 'an absolutely new book in which science is presented in an entirely different form .. into which the revolution ... is consigned in all its developments. Here is the true foundation of the immortal glory that Lavoisier acquired'. On the other, the work also showcased Lavoisier's misjudgements. With regard to oxygen, his inaccurate view of it as acid-generator was prominently highlighted, and his accurate views on it were under-emphasised in part 2. With regard to caloric, although Lavoisier consistently defined it neutrally as 'the cause of heat' thus allowing for a kinetic theory as well as a material theory of heat, as well as for an interchange between the two, much of his description implied an inaccurate material view of heat. He also mis-estimated his own achievements; he said of Part 1 'it is this part alone which contains the totality of the doctrine I have adopted' and partly incorrectly proposed that part 2 was unoriginal. It is reasonable to add another aspect from the contemporary reply to Kirwan: the incorrect claim that in Lavoisier's system 'nothing is admitted but established truths', (Kirwan, 1789, p. xiii).

Wurtz (1869, p.18) had accurately noted that the compositional principles which Lavoisier had formed with regard to oxygen compounds 'were capable of immediate extension to other chemical compounds'. Crosland noted (1973, p. 325) that the oxygen theory had led to a compositional table the vast majority of the results in which remained correct and 'constituted an enormous advance over the ideas of Stahl'. Guerlac's judgement was that the *Traité* was the culminating point of the Chemical revolution.

However, many modern authors have had difficulty in coming to terms with the *Traité* and with the early estimations of it, and a number of them have successively taken several different positions on it. For example, Siegfried indicated in 1968 that Lavoisier began the compositional revolution: when the abandonment of the oxygen theory was accepted 'the system of chemistry based solidly on the concepts of composition and the chemical elements, was not affected in the least'. In 1988 he made a major departure from his former view, by claiming that part 1 of the *Traité* was based on false premises while part 2 was unoriginal, thus Lavoisier's theory did not have a 'consistent central structure' and accordingly was not the foundation of modern chemistry; inconsistently he claimed that Lavoisier's achievements reduced to two of which one was the compositionally-based nomenclature. In 2002 he returned to his 1968 view, noting (p.191) that by joining his 'pneumatic discoveries recounted in part 1 of the *Traité* with the empirical mainstream of the neutral salts under the guise of the new nomenclature in part 2', Lavoisier produced a new compositional chemistry. Holmes also changed position on this, in 1989 (p. 108) echoing the supposed unoriginality of part 2, but changing position in 1995 (p. 48) to indicate 'that part 2 of the *Traité* contained nothing from Lavoisier himself was an unduly modest disclaimer. That it began the transmission of a vast body of earlier chemical knowledge, both empirical and conceptual, into the chemistry of the future, was, however, an accurate assessment'.

## 6. Weight, experiment, theory and philosophy.

There has been much and prolonged debate about what was crucial about the Lavoisier's achievement.

One of the earlier candidates was the measurement of weight. It is a reasonable view that this was important indirectly, in that it emphasised that in Lavoisier's compositional theory the weights of all the items that were affected in a reaction were measurable, whereas phlogiston, if not hydrogen, was not observable nor measurable. It is reasonable to briefly rehearse the historiography. Whereas Dumas (1836) had erroneously ascribed the Conservation principle to Lavoisier, Marcellin Berthelot (1890) already explicitly stated that Lavoisier neither introduced the law of conservation of matter nor was the first chemist to introduce the balance into the laboratory. A summary of the topic given by Guerlac (1961, p. xv) noted that the principle of conservation of matter in chemical reactions had been implicitly used by Van Helmont, was explicitly stated by Jean Rey in 1630, and was quoted by Francis Bacon. From that era on, the quantitative method involving the use of accurate balances and weights was increasingly used and recommended, including by Van Helmont, in Shaw's translation of Boerhave, by Black, and in Macquer's *Dictionnaire de Chimie* (Guerlac, 1961, p. xv). Guerlac suggested that Lavoisier used the balance with 'fidelity and persistence', and this proved crucial even though he did not always use it with 'rigorous accuracy'. Among other authors of that period (Smeaton, 1963, p.56), Partington (1951) suggested that Lavoisier's quantitative method succeeded because of the greatly increased number of substances available to him, especially the new gases discovered by Priestley. Multhauf (1962) noted that whereas the use of the balance by British chemists was occasional, it was used in Europe with dogged persistence for the analysis of minerals, and that this may have influenced Lavoisier. This persistence may explain Kirwan's (1789) statement that Lavoisier overwhelmed his opponents with weight measurements, and this was the impetus behind Kirwan's focus on weights in the first chapter of his 1789 book. Bensaude-Vincent put greater emphasis on this matter, claiming (1993, p. 207) that 'with Lavoisier the balance became the instrument of proof, of persuasion; the balance had to make chemists incline towards an interpretation, remove doubt, eliminate alternative interpretations'; however, in view of her own later identification (Bensaude-Vincent and Simon, 2007) that the very elaborately balanced experiment on water failed to do so, it is reasonable to prefer Guerlac's milder presentation. Most modern authors have credited this aspect of Lavoisier's work in some fashion (e.g. Poirier, 1998; Siegfried, 2002, p. 164). Chang (2009, p. 244-5) however only noted that the emphasis on precise weights and balancing of equations by weight was not wholly new and that Lavoisier's stated proportion of 85:15 for oxygen:hydrogen weight proportion in water was not precise, while (2012, pp. 35-7) arguing that conservation of weight and weight being a proper measure of chemical substances were only assumptions, and that therefore phlogistonists had no reason to accept Lavoisier's constitutional indications.

It is noteworthy that the phlogistonists and Lavoisier to a considerable extent used experiment in different ways – the phlogistonists were primarily interested in finding new phenomena, whereas Lavoisier can be argued to have primarily used experiment to test theories. Priestley on the one hand remarked (1774) that 'speculation is a cheap commodity; new and important facts are what is most wanted' but on the other hand remarked that 'at present all our systems are in a remarkable manner unhinged by the discovery of a multiplicity of facts, to which it appears difficult, or impossible, to adjust them'. Lavoisier (1774b, p. 108-9) exaggeratedly claimed that Priestley's 1774 book was an assemblage of facts, not interrupted by any reasoning; it would have been more reasonable to propose that Priestley's type of ad hoc theorising together with his inadequate discounting of experimental errors, often resulted in

widely inaccurate theories. By contrast, Lavoisier's own view is well exemplified in one of his 1777 papers:

Dangerous though the spirit of systems is in physical science, it is equally to be feared lest piling up without any order too great a store of experiments may .. (result in) nothing but disorder and confusion. Facts, observations, experiments are the materials of a great edifice. But in assembling them, we must not encumber our science. We must, on the contrary, devote ourselves to classifying them, to distinguishing which belong to each order, to each part of the whole to which they pertain (Beretta, 1993, p. 178).

Lavoisier recognised that Stahl's central achievement was the coherent theory, supported by experimental evidence, concerning the similarity of combustion and calcination. Lavoisier reasonably felt that the problems with phlogiston were with the accretion of what would now be called ad-hoc auxiliary hypotheses that had accumulated around it, and with Priestley's views on gases. These were key elements in the disintegration of the theory into a conglomerate of opinions united only by the word phlogiston (Beretta, 1993, p. 21), and as Wurtz (1869, p. 10) noted, disfigured the phlogiston theory rather than saving it. As Lavoisier noted, phlogiston could be adapted to any explanation one wanted to give for it; it was 'a veritable Proteus'. Later suggestions for possible version of phlogiston, including light (Macquer, 1778, pp. 122-4; Perrin, 1986, p. 514), electrons and energy, were each inconsistent with Cavendish's theory, and continued the Proteus analogy.

Holmes (1989, p. 122) argued that Lavoisier was the most innovative experimental chemist of his time, but if so, this innovation was not in terms of finding new phenomena. Hall (1954, p. 332) perceptively noted that Lavoisier was 'less the author of new experiments than the first to realise their full significance' (Holmes, 1998, p. 141). Some authors have attempted to identify one or more crucial experiments in the establishment of Lavoisier's theory, but it is now widely recognised that experiments were not decisive to the establishment of the new theory (Beretta, 1993 p. 215; Bensaude-Vincent, 1993, p. 420).

Concerning affinities, Lavoisier identified the following problems with the affinity tables (Kim, 2003, pp. 341-2): (1) they only showed the results of simple affinities although there existed double triple and further degrees of affinity; (2) heat affected chemical reactions, therefore there would strictly need to be affinity tables for each degree of the thermometer; (3) the affinity tables did not allow for the effect of the attraction of water, which was not simply a passive agent and (4) the tables didn't allow for different degrees of saturation. In his 1783 paper Lavoisier identified that to deal rigorously with all the variables concerned with affinity would require a kind of calculus. In the preface to the *Traité*, Lavoisier separated out compositional issues from issues of affinities; the former had proved to be a fertile field for his investigation, whereas the subject of affinities although 'very useful and necessary' (1789, p. xxi) lacked a solid foundation in fact, which would have taken far too long to put in place and far too big a volume for an elementary study; in addition, affinities were being dealt with by Guyton; nevertheless, most of the tables in section 2 of the *Traité* give substances in order of affinity. Daumas (1955) noted that 'in his more specialised memoirs, Lavoisier invoked affinities in appropriate contexts, and he himself constructed a table of the affinities of oxygen for twenty-five different substances; he was accurate in his assessment of the difficulties surrounding affinity theory'. Despite Lavoisier's actual statements and Daumas' endorsement, Kim claimed (2003, p. 359) that 'historians have mostly taken Lavoisier's words in the *Traité* at face value and disregarded affinity as an important subject'; she nevertheless also claimed that (2003, p. 357) 'the Arsenal group conceptualised the constitution of chemical bodies as an interplay between heat and affinities'.

Concerning previous philosophical views on the subject, as noted by Musgrave (1976), Lavoisier's theory was not established by induction, nor did theory change follow some form of 'sufficient justification'; in line with Duhem's thesis, anomalies with each theory could be rationally ignored on the grounds that they related to some relatively unimportant auxiliary hypothesis, not to the main theory. It has been argued (Pyle, 2001; Blumenthal, 2011) that few of Kuhn's main structural ideas from his (1962) apply in this case, except the view that a theory is only abandoned when out-competed by another theory. As Musgrave (1976) noted, Lakatos' views on competing research programmes work well in many respects with regard to this period; however, Zahar's criterion of novel fact appears insufficient to explain the change in this case, even if one takes into account the implicit prediction of substances not previously found experimentally in the *Nomenclature* and the *Traité*. Laudan's view that a revolution occurs when a research programme develops to the point that the scientific community begin to consider it seriously as a candidate for adherence would apply well in this instance, although it can reasonably be proposed not to be a sufficient definition of revolution even in this instance.

Margolis' (1993) theory that revolutions are partly to do barriers to progress formed by existing theories, increasing the more usual obstacle to scientific change presented by the inner strengths of explanatory theories (Meyerson, 1991), is also useful in this instance. The word phlogiston was so central to its theory, and so well established, that adherents were reluctant to abandon it since the whole theory went with it (as Fourcroy noted); that then became a barrier to progress. This barrier explains the solid public opposition that Lavoisier encountered: as Fourcroy (1797, vol 3. P. 541) stated, 'from 1777-1785, in spite of great efforts and numerous memoirs of Lavoisier, he was truly alone in his opinion'. Guyton in his second foreword presented 'the doctrine of phlogiston as an error, formerly fertile, but now transformed into a dogma, into a religion which was superstitiously respected instead of judged' (Bensaude-Vincent, 1993, p. 263) and presented Lavoisier as 'the first to interrupt this long cult'. The phlogistonist Sage wrote a public letter to Lavoisier requesting 'allow me, my dear colleague, to have my religion, my doctrine, my language' (Beretta, 1993, p. 224); the sense of loss on the eventual abandonment of phlogiston was also experienced by many phlogistonists, (Siegfried, 2002, p. 196; and Boantz and Gal, 2011, p. 25), and the difficult process of adjustment to the new system was illuminated by Black in 1791 when writing to Lavoisier (Perrin, 1982, p. 163). As Lavoisier's theory obtained increasing numbers of adherents, counter-accusations of religious dogmatism were made against it by the phlogistians, a process that has continued among some recent historians, despite their recognition of the very varied take-up of the system.

In order to improve the relationship of Lakatos' theory to this period, a reasonable option is to replace Zahar's criterion of novel fact with the epistemic virtues, taken together as a group. The list given by Bird (1998, p. 263-4) will be used here, together with a criterion of accuracy.

The expansion and consolidation of Lavoisier's theory from 1783 to 1789 may be proposed to have involved the following epistemic virtues: the theory achieved greater simplicity and consistency, in terms of the reduction in the number of simple substances and the describability of a very large number of substances as compounds of the simple substances, and named as such rather than having new ad-hoc names. It achieved fertility, particularly given that the system could be used to predict new compounds which could be expected to exist. All the elements of the compositional theory were accessible to quantitative analysis. The theory achieved observational nesting – that is, it could substantially cope with observations with which the previous theory could cope. All these virtues were in evidence by 1790. Additionally, the theory could be seen by 1815 to have achieved some degree of smoothness, in terms of the jettisoning of the heat-as-substance and oxygen-as-acid-generator hypotheses while leaving the rest of

the theory extant. With the latter exceptions and some other exceptions, the theory has subsequently proved largely accurate.

By contrast, the later phlogiston theory fell down with regard to the following epistemic virtues: phlogiston itself, once the option of identifying it with hydrogen was discarded, proved not available to even indirect observation and could not thus be analysed quantitatively; this is also why the phlogistic compositional theory proved infertile and incapable of development. Even if all the version of the theory prior to 1784 are disregarded, the later theories never managed consistency, and as already noted the later suggestions for phlogiston were inconsistent with Cavendish's theory. The theory completely failed in terms of smoothness, since the problem concept could not be discarded without discarding the whole theory. The actual phlogiston theories have proved inaccurate.

Authors stressing Lavoisier's achievement in terms of theory have included Kopp (1842, v.I), Whewell (1847, v. 3, p. 152), Toulmin (1957, p. 214), Guerlac (1961, p. xviii-xix), Siegfried and Dobbs (1968, p. 275), Verbruggen (1972, p. 66) and Poirier (1998, p.113/4).

## 7. The take-up of Lavoisier's theory.

The take-up of Lavoisier's theory tended to be simplistically represented in the early historiography, treating the take-up of the theory in monolithic fashion – for example, Thomson and Partington stated that Lavoisier's theory was swiftly taken up after 1785. Perrin (1988b) produced a classic study of the take-up, showing that there were very few adherents before 1784, but suggesting who 'converted' and when after that date. Whereas Perrin properly noted several caveats concerning what constituted 'conversion', he just used a single category of 'conversion' in his table, which thus remained somewhat misleading. However, as he started to identify, there was actually a far more varied picture, to understand which it is helpful to look at some of the participants individually.

Berthollet was among the first to give public support to Lavoisier's theory, in early 1785, prior to Lavoisier's '*reflections on phlogiston*'. His testimony concerning his own conversion, and the scientific reasons for it, is best stated in a proselytising letter (4 May 1785) to Guyton: 'while M. Lavoisier could not give any explanation of the formation of inflammable air and while he had false ideas on the nature of nitrous acid, I defended the theory of phlogiston ... but the decomposition of water, (etc)... have substituted the data of experiment for a useless hypothesis'. Kim noted (2003, p. 525, n. 85) that 'Berthollet continued with the mission until Guyton's conversion in 1787'. However, his adherence to Lavoisier's ideas was not total – he retained doubts about Lavoisier's theory of oxygen as acid-generator – but these were in place alongside his general enthusiasm and proselytising zeal for Lavoisier's ideas.

Guyton had a longer track record of publications which adhered to the phlogiston theory, and Lavoisier's 21 April 1773 memoir to the Academy had reached opposite conclusions to those in Guyton's *Digressions*; this no doubt partly explains why it took Berthollet two years to 'convert' Guyton, even after the death in 1784 of Guyton's supporters Bergman and Macquer and the advance in Lavoisier's theory. Guyton's long opposition and arduous route to 'conversion', plus his recognition of the semi-religious character of phlogiston theory, perhaps explain his Manichean tribute in his second preface to the *Dictionnaire de Chymie* (1789), which stated 'Before Lavoisier confusion and darkness reigned over chemistry; after him there was light and a smooth road leading to truth'.

The crucial later participant in the debate was Humphry Davy, the stages of development of whose views are particularly interesting. Davy firmed up the theories that heat involved motion and that acidity did not depend on the presence of a property-bearing principle, (Siegfried, 1964, p.118). An

interim stage in his research process occurred following the electrolysis of water by Nicholson and Carlisle in London (1800), the identification of the perceived problem that oxygen was given off at one electrode and hydrogen at the other, and the hypothesis by Ritter in Germany that what was produced was negatively charged water at one end, and positively electrified water at the other. Davy showed in his excellent Bakerian lecture in 1806 that what was produced in this experiment was gaseous matter alone, oxygen and hydrogen, (Davy, 1807; Golinski 1992, p. 213). He then spent part of five years (1807-12) in an 'extensive flirtation' with phlogiston, which proved infertile, and from which he 'emerged rapidly' (Knight, 1978, p. 136) and produced (1939, vol V p. 434-5) a near-recantation by noting that 'the chemists in the middle of the last century had an idea that all inflammable bodies contained phlogiston or hydrogen. It was the glory of Lavoisier to lay the foundations for a sound logic in chemistry by showing that the existence of this principle, or of other principles, should not be assumed where they could not be detected'. The reason given was not a fully accurate reflection of Lavoisier's work, but it identified the main problem with phlogiston, that it had in practice proved unobservable.

The problems of labelling are illuminated by Chang's (2010, p.64) listing of Davy as a new anti-Lavoisian and of Berthollet as a 'fence-sitter'. Kim (2003, p. 337) inconsistently claimed that 'instead of talking about 'conversion' we should characterise the mid 1780s as a period of intensified interaction among the four authors of the nomenclature' – however, the above matters suggest that it would be far more accurate to talk about 'conversions' plus a period of intense interaction among the four starting in 1787.

## **8. A very brief perspective on the historiographical positions.**

There was difficulty in accurately judging Lavoisier's achievement from the start. Lavoisier himself tended to emphasise his unanimity with collaborators when making points concerning the opposition, but otherwise to stress his personal achievement; this ambivalence was partly echoed by Fourcroy. Lavoisier also mis-estimated his own achievement, over-valuing his oxygen and caloric theories and under-valuing his impact on compositional chemistry. Guyton's second foreword was composed after a period of tumultuous change in his own opinions, and although he accurately foresaw that Lavoisier had produced a wide body of lasting chemical theory which was a germ of the chemistry that would later develop, he over-estimated how much of Lavoisier's theory would prove lasting. However, these matters can reasonably be proposed to have been due to lack of perspective rather than an overriding cause of distortion. The obvious early case of major deliberate distortion, by de la Méthérie (1789, p. 22-40) remains somewhat noteworthy if only because it provides the first evidence concerning quite how much fact had to be left out in order to produce an illusion that Lavoisier had been unimportant. Thomson's (1830, v. 2, p. 134) comment was 'his views ... in all probability rather served to promote than injure the cause' of Lavoisier.

Thomson's accounts offer a proper detailed emphasis on the science and a relative lack of extraneous issues compared with most later nineteenth century accounts, so that it is worth summarising his position: he stated concerning Lavoisier's achievement of a general theory that 'it is not those who collect the stones and the timber and the mortar, but he who lays the plan and shows how to put the materials together, that is in reality the builder of the house' (Partington, v.3, p. 376); concerning combustion Thomson (1802, v.1, p. 596) noted that Lavoisier's theory that during combustion, oxygen combines with the burning body, 'has connected and explained a vast number of facts formerly insulated and inexplicable'. Thomson (1802, v.1, p. 598) inaccurately claimed that Lavoisier had 'destroyed the



existence of phlogiston altogether'. He correctly noted Lavoisier's use of the definition of caloric as 'the cause of heat', independent of particular theory of heat; he noted Lavoisier's extensive achievements in documenting the compounds of oxygen including the oxyacids, although noting that the theory of oxygen as the universal acidifying principle was incorrect. He praised the new chemical nomenclature as quoted above, and stated (1830, v. 2, p. 114) that Lavoisier 'saw at a glance the vast importance' of the discovery of the composition of water for the establishment of his new theory. The other nineteenth century accounts which are most notable for inclusiveness and attempted balance were by Kopp (1843 and 1873).

It is not unreasonable to propose that the deliberate bias in many accounts by leading French and German authors, occurring for nationalistic reasons or reasons of disciplinary or personal advantage (Meinel, 2005; Kim, 2005), was initiated by claims exaggerating Lavoisier's achievement by Dumas (1836) and continued by Wurtz's (1869) first sentence that 'Chemistry is a French science: it was constituted by Lavoisier'; while Wurtz's English translator, Henry Watts, noted that despite that claim Wurtz had 'habitually done full justice to the labours of chemists belonging to other nations', the first sentence particularly aroused German ire. Subsequent French accounts were at any rate less biased than Dumas's, including the first full book on Lavoisier's science by Berthelot (1890); Duhem (1916) correctly credited Stahl and correctly noted that Lavoisier incorporated partial realisations by his predecessors into a more coherent view; however, both retained some over-emphasis on Lavoisier. In response, the Germans (e.g. Liebig, 1851) tended either to nearly completely ignore Lavoisier's work, as de la Méthérie had, or to minimise the difference of his account from that of Stahl: Volhard (1870) claimed that Lavoisier was a dilettante plagiarist, and he and Ostwald (1906) claimed that Lavoisier only inverted Stahl's theory, and thus continued Stahl's tradition (a view resurrected by Gough, 1988). Metzger inverted this nationalistic pattern by doing arguably her most illuminating work on Stahl (1926 and 1927) and under-estimating Lavoisier's contribution (1935).

Early twentieth century exceptions to nationalistic bias included Meyerson, who also correctly noted (1991, p. 562) that Lavoisier 'has no need of being aggrandised, since he is certainly one of the most authentic great men humanity has ever produced in any field' (Bensaude-Vincent, 2005, p. 632).

After 1950, it is reasonable to note a predominantly correct position with a germ of imbalance that was produced by Partington (1961, v. 3, pp. 376-7). On the one hand, he quoted Thomson's house analogy, stated that Lavoisier's main contributions were his recognition and experimental proof that combustion, calcination and respiration involve the taking up of the 'ponderable' part of oxygen gas, his results on the composition of oxyacids and of atmospheric air, his clear statement (from the results of Cavendish) of the nature of the components of water, and the ruin of the phlogistic theory and the setting of chemistry on the road it has followed ever since. On the other, he over-emphasised as 'major mistakes' Lavoisier's 'assumptions' that all acids contain oxygen, that combustion 'occurs only in oxygen gas' and that the heat and light evolved in combustion come from an imponderable part of oxygen gas.

While some of the more recent historiography has been notably informed and balanced (e.g. Beretta, 1993), it has been more widely the case that the subsequent historiography 'rode madly off in all directions' (Leacock, 1911, p. 30). Perrin (1988a, p. 79) noted that 'alternative interpretations have proliferated (and) practitioners talk at cross-purposes; ... the parable of the blind men and the elephant comes to mind: each of us has touched a different part of the beast and may be tempted to take it for the whole'. Holmes (1997, p.1) noted the potential question whether 'historians are unable to settle anything'.

Bensaude-Vincent (1983a and 1983b) critically suggested that previous French writers - Guyton, Dumas, Wurtz and Berthelot - had over-emphasised Lavoisier; she concluded (1983b, p. 196) that 'history is perhaps the privileged location where the imagination of a science is constituted, where new

religions are constructed, the faith of modern times'. Her work contributed 'imagination' of her own, involving divergent sets of claims. One set tended to under-estimate Lavoisier's achievement, claiming (1993, p. 33) that he in the 1770s used 'revolution' with 'no question of rupture or reversal' (but e.g. see Perrin, 1989, p.24), that there was no named theory to 'convert' to from phlogiston (p. 256), and that 'if the nomenclature was mostly adopted ... that did not signify a massive conversion to the antiphlogistic theory' (1993, p. 284). Despite her previous criticism of other writers inflating Lavoisier's impact, her second set of claims also inflated Lavoisier's impact while ignoring obvious contrary evidence. Despite the previous proposals for revised nomenclatures, despite the fact that the *Nomenclature* used old words for simples in most cases, and despite the presence in the *Nomenclature* of thoroughly detailed dictionaries giving old and new names, she claimed that Lavoisier made past and present incommensurable and untranslatable (1993, p. 423; c.f. Kuhn, 1962). Despite the previous points, despite Lavoisier's (1774a) work giving a detailed history of previous pneumatic chemistry, and despite the omission of history becoming a usual practice in textbooks (c.f. Berzelius, 1825; Meinel, 2005, p. 156) she claimed that he 'invented' revolution as brutal rupture (1993, p. 233 and 423) and that he divided chemistry between a forgotten past and an eternal present (1993, p. 233). She noted that 'in our whole culture, the balance is the symbol of justice and equity' (1993, p. 202), and then on the basis of Baumé et al's claim that the previous theory could not be reversed 'in a day', and despite the fact that Lavoisier's challenge had taken 15-20 years (c.f. Wurtz, 1869, p. 2; Siegfried, 1989, p. 31; Beretta, 2005 p.11), she claimed that imbalance defined Lavoisier's revolutionary protest (1993, p. 252), and proceeded to claim that for a principle of conservation he substituted that of amnesia (1993, p. 423). Whereas she had noted Guyton's view of phlogiston as 'religious' (1993, p. 263) and stated there was a wide variation between chemists as to which among Lavoisier's theories they adopted (1993, p. 273), she nevertheless claimed that phlogiston was (part of) the 'natural philosophy of the enlightenment' while Lavoisier produced a religion (1993, p. 423). Her final claim was that he invented not just modern chemistry but modernism itself (1993, p. 423). Noteworthy views contrary to some of the above, have been put by Kim, (2005) and Klein and Lefèvre (2007). In the context of all the above claims, it may seem somewhat odd that Bensaude-Vincent was insistent that Lavoisier's revolution was 'an event integral to French culture' (1993, p. 284).

Another unbalanced negative view was given in Siegfried's 1988 *Osiris* article, in which he omitted many of the achievements recognised by Partington, incorrectly inflated Partington's view (without supporting detail) into the claims that Lavoisier's false 'central assumptions' were the caloric theory of heat and the oxygen theories of combustion and acidity; part 1 of the *Traité* was based on false premises while part 2 was unoriginal; thus Lavoisier's theory did not have a 'consistent central structure' and accordingly was not the foundation of modern chemistry. Inconsistently he claimed that Lavoisier's achievements reduced to the compositionally-based nomenclature plus the empirical facts on combustion. This view went away from his reasonable view (Siegfried and Dobbs, 1968) that Lavoisier began the compositional revolution as well as completing the overthrow of phlogiston; he had then stated that when the abandonment of the oxygen theory was accepted 'the system of chemistry based solidly on the concepts of composition and the chemical elements, was not affected in the least'. In 2002 he implicitly retreated from his 1988 claims and returned to his 1968 view, now agreeing the dramatic revolution produced by Lavoisier's anti-phlogistic chemistry, with its reversal of the relative simplicity of composition of the bodies involved, 'the long-range consequences of which made possible the present organisation of all chemistry on composition', and noting that part 1 of the *Traité* included Lavoisier's pneumatic discoveries, which also led to the novel perspective on compounds in part 2.

One element in Siegfried's change of views after 1988 may have reflected Perrin's criticism of his 1988 claims, noting 'the lasting contributions of Lavoisier's understanding of oxidative processes, his articulation of a viable system of chemical composition, his establishment as working principles of the conservation of matter and the operational definition of element, his formulation of the idea of a chemical equation, as well as his founding investigations in thermochemistry, organic analysis and physiology' (Perrin (1988) p.79).

Kim coupled Siegfried's 1988 claims with the claim that historians had not been able to link the Chemical Revolution to the nineteenth century development of atomism (2003, p. 391; 2005, p. 168), although Siegfried had briefly done that in 1968 and his 2002 book had just elaborated that while amending the emphasis of his 1988 claims. She ignored nearly all the original aspects of the Nomenclature and claimed that it was 'not his' (2003, p. 391). While in her text praising Lavoisier for a positive attitude to affinity (her special subject), in her conclusions she claimed that Lavoisier had a narrow focus on studies of air and heat, whereas Guyton had a 'comprehensive vision' based on affinities (2003, p. 391).

The full range of Chang's work on the subject in and since 2009 cannot be adequately covered in a paper of this length. However, the matters outlined in this paper have illustrated the incorrectness of his claims (2009, p. 240) that 'Lavoisier was quite wrong or useless, whiggishly speaking' and (2009, p. 239) that 'Lavoisier's theory was just as wrong as advanced versions of the phlogiston theory'. The central relevant point of Chang's recent book is that in 1784 there were two theories, one being Cavendish's theory with water as an element, modified by phlogiston (2012, p. 6). Chang claims that phlogiston was 'killed' too soon (2012, p. 14), that it could have been profitably retained (2012, p. 42), and that the 'truly pluralistic challenge' would be to find and develop a science based on water as an element, with phlogiston (2012, p. 13). To be consistent with his normative recommendations, he would need to develop 'systems of practice' (2012, p. 15) which would be as incommensurable as possible (2012, p. 217-8), facilitating the operational development (2012, p. 153) of testable hypotheses, and resulting in multiple conserved incommensurable systems of practice (2012, p. 224). In practice he does not follow these recommendations: he takes the equation  $4\text{H}_2\text{O} + 4\text{e}^- \rightarrow 2\text{H}_2 + 4\text{OH}^-$ , claims that he can ignore the  $4\text{OH}^-$  as a 'by-product' (2012, p. 210) and that this is not 'a great big cheat' (2012, p. 210), and claims that this rehabilitates Cavendish's view that hydrogen gas was phlogisticated water. Among other problems, this procedure is not operationalist, does not involve a 'system of practice' in the sense he uses elsewhere, and does not treat water as an element.

## 9. Conclusions

The modern trend towards recognition of the achievements of scientists other than those who are most eminent, including the losers in major arguments, is perfectly reasonable in principle. However, in the case of Lavoisier it has led to over-estimation of the achievements of others and unwarranted denigration of Lavoisier.

Lavoisier's central (but not only) achievement in chemistry stemmed from scientific practice involving theoretical development and experimental testing, leading to the production of a coherent body of theory, of which the inaccurate aspects, although resulting in considerable confusion at the time, were discardable, leaving a remaining consistent wide largely accurate theory, which set chemistry on the road it has followed ever since. The principal components of this remaining theory included (but were not limited to) the following:

Air is not one of four indivisible elements (earth, air, water and fire) but is a mixture, of which the major components, at least when occurring in solid form, are simple substances. The simple substance that enters into calces, and combines with a combustible during combustion, is also the component of atmospheric air that supports life, and is in a proportion of roughly 1:4 relative to the parts that do not support life. Metals are simple substances and calces are compounds of metals and (the ponderable part of) 'eminently breathable air'. Many single substances will combine with different proportions of 'eminently breathable air' to form different oxides or different acids. Acidity increases dramatically as the proportion of oxygen in an oxyacid increases, and nearly all then-known acids contained oxygen. The third principal component of most salts, in addition to the base and radical, is oxygen. Water is not an indivisible element but is, broadly speaking, a compound of two simple substances. Original aspects of the jointly-authored *Nomenclature* included: the method of naming being proposed to be more important than the specific nomenclature, the inclusion of simple substances that are components of atmospheric air and of water, a classification of oxides which correctly indicated their component simple substances and how much oxygen the particular oxide contained, and a classification of neutral salts that for the first time managed to indicate via two words the three simple substances of which they were composed and specified via suffixes that the salt contained oxygen and relatively how much oxygen the particular salt contained. The *Traité* included a list of simple substances that was largely correct and was the precursor of the modern table of elements, and lists of many compounds, of many of which the constituents for the first time largely accurately identified what have later been recognised as chemical elements. This theory became the start of modern compositional chemistry. Lavoisier's research with Laplace on heat was formulated to be independent of whether heat was a substance or was motion, dealt with the real issues of latent and specific heat, and laid the foundations of thermochemistry.

The rational adoption of Lavoisier's theory by other chemists involved comparison with the competing theory, and can be proposed to have involved the following among other considerations: (a) Lavoisier's theory, although encumbered by a number of aspects which were later discarded, had a core which was and continued to be fertile, particularly in enlarging the scope of chemical compounds it included while also representing them more simply and consistently; (b) no consistent version of a theory including phlogiston, water and the known varieties of air had been produced, and (c) once the identification of phlogiston with hydrogen was abandoned, no-one managed to identify and undertake a practice of experimentation explicitly related to testing it. Despite the latter considerations, 'Lavoisier was ... the only chemist in Europe who systematically and repeatedly questioned the existence of phlogiston, who published several memoirs against it, and who set forth a theory alternative to it' (Beretta, 1993, p. 183).

As Holmes (1989, p. 111) stated, 'Lavoisier was the most probing, the most systematic, and the most critical theoretician who had up to his time entered the field of chemistry ... he did impart to chemistry a more inclusive, more coherent and more durable theoretical structure than it had previously possessed'.

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